

Chicago

"Today is the first day of the rest of your life," says an upbeat homily. This is the way I felt on my first day as college teacher, when I faced forty men, to whom I was engaged to teach differential calculus. They all wore a little yellow lapel pin (the "ruptured duck"), identifying them as war veterans. I, too, wore one. These men were using their opportunity to get a post-secondary education under the (U.S. veterans) GI Bill of Rights. They, too, stood at the beginning of something. I was sure they felt as I did.

I, too, took advantage of the GI Bill and enrolled again at the University of Chicago. I thought I would get another doctorate, perhaps in some biological science. But it turned out the University did not award more than one Ph.D. I took the courses anyway, developing a sort of addiction or a hoarding compulsion. I told myself that hoarding knowledge could not be as reprehensible as hoarding money or power. Money and power were "conservative commodities": the more one gets of them, the less others get. But if A gives B a piece of knowledge, B is enriched while A is not impoverished. Iago called attention to the grim reverse side of this condition: "...he that filches from me my good name robs me of that which not enriches him and makes me poor indeed."¹

So I felt self-righteous about hoarding knowledge, especially since my longing to impart and disseminate knowledge was so keen.

There was another sense in which I felt that my "return from the wars" was the beginning of "the rest of my life." I got married.

Gwen was a friend of Mrs. Hayakawa (both had been active in the cooperative movement). We first met at a New Year's Eve party at the Hayakawas, at which I was with another woman and she was with another man. I got into a heated political discussion with her escort. During the fifty years of our marriage Gwen often needled me about not remembering her when we met again at the Hayakawas almost a year later on Saturday, December 4, 1948. As soon as I saw her walk into the room, I knew that I would marry her.

Gwen lived in Madison, Wisconsin at the time, working in the School for Workers at the University of Wisconsin. She still kept an apartment with a friend on the Near North Side of Chicago, where she stayed when she came to town. I invited her to the opera on the following Saturday. We saw Eugene Onegin. Her

next visit was during the Christmas week. We met again at the Hayakawas on December 26 and every day thereafter. I proposed on New Year's Eve and was accepted. We have gone through the same ritual at 6 P.M. on December 31 every year since then.

Gwen stayed in Madison until summer. We spent our honeymoon (six months after our marriage) cycling from Zurich to Venice. As we pushed from Altdorf up to the Gotthard Pass, I told Gwen about Suworov's expedition against Napoleon in 1799 (which I remembered from the Russian history lessons of my childhood). That crossing was compared to Hannibal's feat two thousand years before. Just as I was telling the story, we came around a bend and saw a huge carved inscription on a rock in Russian commemorating the expedition.

Gwen joined me at the University of Chicago where she enrolled as a graduate student, worked in the Industrial Relations Program and directed a training program for union officers. We shared a political outlook, a thirst for knowledge, and delight in family life, giving concrete meaning to the ancient Russian proverb, *muzh da zhena—odna satana* (man and wife are one devil). Why devil? I guess just because *zhena*, *odna*, and *satana*, all accented on the last syllable, rhyme—another confirmation of the Sapir-Whorf-Korzybski hypothesis: our language does our thinking for us.

Knowledge that interested me most turned out to be knowledge of the second order, as it were: knowledge of self, knowledge of knowledge. How do we know what we know? How can we tell truth from falsehood, from delusion, from nonsense? In short, theories of cognition became central in my preoccupation: general semantics, philosophy of science, psycholinguistics, general system theory. Under Rashevsky's tutelage I explored the range of application of mathematical methods outside the physical sciences: to individual psychology, theories of mass behaviour, description of social structures, inferential statistics (measures of reliability of knowledge), etc.

All these fields have to do with criteria that justify beliefs in what is. Of equal, perhaps greater importance are fields having to do with criteria justifying beliefs in what ought to be: primarily ethics.

Of greatest importance to my way of thinking then and subsequently to this day have been criteria that link the two kinds of belief. The linkage, as I began to see it, is reflected in the four parts of my book on *Semantics* published in 1975: Man and His Language, Symbol and Meaning, From Meaning to Knowledge, From Knowledge to Responsibility.

Another such chain connects normative and descriptive decision theories, that is, theories of how a "rational" actor ought to choose among available alternatives and theories of how real actors choose. Preoccupation with game theory led me to scrutinize the very concept of rationality, in particular, the sharp distinction between individual and collective rationality revealed in the analysis of so called non-zero-sum games, in which what some win others do not necessarily lose. The relevance of these findings to the increasingly widely recognized global problems became a constantly recurring theme in my preoccupation and expression.

The most formidable obstacle to the solution of these problems appeared to me to be the Cold War and its ramifications: the nuclear arms race, ideological polarization, the cancer-like spread of war-related institutions through the entire fabric of both superpowers. My book, *The Big Two* (1971) is devoted totally to this theme, which is also a major theme in *Fights, Games, and Debates* (1960), *Strategy and Conscience* (1964), and *The Origins of Violence* (1989).

In what follows I will attempt thumbnail descriptions of several fields of knowledge (or, more properly, speculation) that determined the crystallization of my outlook on the way things are and the way I think they ought to be, which is the principal theme of this book.

My First Paper

Rashevsky's group, eventually called Committee on Mathematical Biology, was housed in a flat of an old house on Drexel Avenue just off the University of Chicago campus. Rashevsky taught a course based on his monograph, *Mathematical Biophysics* and presided at weekly seminars, where members of the group (five or six young men and a young woman) presented papers subsequently published in the *Bulletin of Mathematical Biophysics*. At the time they and Rashevsky were the only contributors. The journal, now called *The Bulletin of Mathematical Biology*, has world stature today.

Soon Rashevsky offered me a part time assistantship with his group, and I accepted, splitting my time between it and my job at the Illinois Institute of Technology. My first task was to proof-read a paper of Rashevsky's entitled "A Problem of Mathematical Biophysics of Interaction of Two or More Individuals Which May Be of Interest in Mathematical Sociology."

The paper gave me an idea. What would happen if each individual pursued his own "interest," and, in doing so, interacted with another, who also pursued his own interest? There are two ways of answering this question. To see "what would happen" one could observe a situation of this sort. Or one could postulate certain principles governing the dynamics of the situation and deduce the implications of these principles. The first method is based on principles of empirical science, putting direct questions to nature, as recommended by Francis Bacon in the seventeenth century. The second method is the method of mathematics, celebrated by Plato and Descartes. In textbook descriptions, "the" scientific method is portrayed as a cycle in which deduction (mathematical derivation) alternates with observations, which either confirm or disconfirm the deduced predictions of "what will happen." Rashevsky was concerned exclusively with deduction, and for that reason the Committee on Mathematical Biology remained on the fringes of the scientific establishment of that time. Alvin Weinberg² pleaded with Rashevsky jokingly to let us put on white coats so that we would at least look like "scientists," but Rashevsky would have none of it. He insisted on a special niche for mathematical biology, which, he was convinced, would some day attain a status comparable with that of mathematical physics.

So my aim was to construct a mathematical model of two interacting individuals, each pursuing his own interest. First, the notion of "pursuing one's own interest" had to be defined in a way permitting mathematical deduction. This could be done by identifying the "pursuit of interest" with maximization of some quantity. In economic contexts, this might be maximization of profits by a producer of marketable goods. In a biological context it could be the maximization of fitness (reproductive advantage). From the point of view of the mathematical model builder, the concrete context does not matter. Only the mathematical form of the assumptions matters.

To fix ideas I assumed that each of the individuals produced some nurturing substance that both individuals shared, whereby the amount of "satisfaction" derived by an individual was represented by the difference between two terms. One was a logarithmic function of the amount of nutrient received; the other a linear function of the effort expended.³ Thus, the respective satisfactions of the two individuals were denoted by $U_1(x,y)$ and $U_2(x,y)$, where x and y are the amounts of nutrient produced by the two individuals respectively.

Each individual adjusted his own effort so as to maximize the utility accruing to him, given a particular level of effort of the other individual. To find the point of equilibrium of the maxima attainable by such adjustments, we set the rates of change of $U_1(x,y)$ and $U_2(x,y)$ with respect of x and y respectively (what the economist calls marginal utilities) equal to zero.

The point (x,y) at which both rates of change of effort vanish represents an equilibrium in the sense that neither individual can increase his satisfaction by either increasing or decreasing his effort, if the other does not.

Two illuminating results can be derived from this model, which represent a sort of "theoretical harvest." By this I mean an understanding of the nature of the phenomenon represented by the model. One result relates to the nature of the equilibrium represented by the point (x,y) . The other is an implication of the individuals' independent attempts to maximize their respective satisfactions. Let us consider the nature of the equilibrium first. Physical equilibria are characterized as stable or unstable. A case of a stable equilibrium is represented by a bead resting on the bottom of a hollow hemisphere. If it is displaced, it will move generally downward and will eventually come to rest again on the bottom. An unstable equilibrium is represented by a bead resting on the top of the hemisphere. A slightest displacement will send it rolling down, and it will never come to rest in its original position.

Similar examples of stable and unstable equilibria are observed in a wide variety of situations. The temperature of a match is initially in equilibrium with that of its environment. If the match is rubbed against a rough surface, the temperature will rise. If it rises only slightly, it will return to an equilibrium with the environment when the rubbing stops. But if it rises beyond a certain threshold, the match will ignite and will no longer return to its original state.

Certain body states of living creatures are kept in equilibrium, for example, concentrations of substances in the blood or body temperature of warm-blooded animals. These quantities oscillate around equilibrium states. As long as the

oscillations are not too large, the concentrations or the temperatures tend to return to their equilibrium states after disturbance. However, once a deviation becomes sufficiently large, the equilibrium is no longer restored. For example, the organism dies. Death is an irreversible event.

The equilibrium of the system comprising the satisfaction-maximizing individuals can also be stable or unstable. If it is completely unstable, the slightest deviation will be magnified rather than diminished. That is to say, the efforts of one individual will keep increasing, while that of the other will keep decreasing, until the latter will become "parasitic" on the other. Which of the individuals will become the parasite depends on the direction of the initial displacement from the equilibrium.

If this is indeed the case, that is, if parasitism can develop in an arrangement of this sort, the question naturally arises under what conditions this will occur. The mathematical model suggests an answer. The system will be unstable if the product of the portions shared is greater than the product of the portions retained. One may find this result intuitively acceptable and regard it as an indication that the model does capture some aspect of a situation of this sort. The other theoretical spin-off of the model is revealed by the observation that the amounts of satisfaction accruing to the individuals at equilibrium are not the largest that can be attained in this situation. Both individuals can benefit if their efforts are interdependent, for example if each exerts an amount of effort proportional to the amount exerted by the other.

In sum, my first effort at constructing a mathematical model led me to the realization of two principles encountered in a great variety of situations. First, situations involving interactions between individuals pursuing their respective interests can be stable or unstable. In a stable situation some sort of equilibrium can be expected to be attained, in which the system will persist. If it is unstable, the equilibrium, although theoretically possible, will not persist. The system will move away from it and go toward one or another extreme. In the situation depicted above the sharing of a resource "degenerates" into parasitism, whereby which of the individuals became parasitic on the other depended more or less on chance, that is, on the direction in which the system happened to move from the precarious (unstable) equilibrium. Second, it turned out that when each attempts to maximize his own satisfaction, the individuals fail to achieve the levels of satisfaction they could have attained if their efforts were interdependent. In other words, the result pointed to a "cooperative dividend," a concept that was to dominate my thinking about the human condition for the rest of my life.

Mass Behaviour

Another theme around which much of my early work at the University of Chicago revolved was mass behaviour. Rashevsky's interests switched from mathematical biophysics to more general contexts of mathematical biology, and soon afterward to mathematical psychology and sociology, that is, to constructing mathematical models of human behaviour—individual or in the mass. The latter context interested me more, because the phenomenon of mass behaviour appeared to me to resolve the age-old controversy between "determinism" and

"free will." Surely there is no "objective" evidence supporting either position. To "prove" determinism of human behaviour one would have to show that the behaviour of an individual can be predicted even if he is aware of the prediction. But unless the individual is constrained, he can always falsify the prediction at least in some contexts. On the other hand, if an individual attempts to demonstrate "free will" by predicting his own behaviour and then fulfilling the prediction, there is no way of refuting the conclusion that his very "prediction" was determined by his state at the time it was made. I had long decided that the controversy was sterile as are a great many "metaphysical" arguments of this sort. It did make sense, however, to investigate conditions under which human behaviour can be predicted more frequently and with greater accuracy. It turns out that human behaviour in the mass can often be predicted much more accurately than the same behaviour performed by an individual.

This principle was amusingly demonstrated in the late 1940's, when television appeared in a large majority of American homes. Within two or three years practically every American household was equipped with TV. It was noticed that during the so called "prime time" hours, water pressure in city mains suddenly dropped for two or three minutes every half hour on the half hour. An explanation readily suggested itself. When the programs changed, people went to the bathroom. It became clear that while every individual had the "free will" to go or not to go to the bathroom, in the mass people's behaviour might be governed by "laws." Moreover, it became clear that such laws are not like the classical deterministic physical laws but rather manifestations of statistical effects. Here, then, I thought, was a basis for mathematicized biological and social sciences.

Epidemics and Rumours

Rashevsky's book, *Mathematical Biology of Social Behaviour* appeared in 1951. In Part I, there were still some allusions to brain functions as determinants of behaviour, but that is as far as "biology" went. The rest of the book was devoted to formal mathematical models, explicitly or implicitly statistical, of the development of social hierarchies, of imitative behaviour, of motivation, imitation, learning, and so on.

I imitated him. In constructing models of spreading epidemics I noted that the spread of information, of rumours, or of fads may follow the same patterns. There was no opportunity to test these conjectures. But that did not worry me. *The Bulletin of Mathematical Biophysics*, edited by Rashevsky, was practically a guaranteed publication outlet. Rashevsky was not a careful editor. He was enthusiastic about spreading the methods of mathematical model building into every nook and cranny of the social sciences, regarding them as generalizations of his mathematical biology, which, in turn, he regarded as a generalization of mathematical biophysics. In this way a gross error in my third paper on interaction between two individuals went unnoticed and became a source of embarrassment to me. And it is hardly a consolation that no one noted the error. This only indicates that hardly any one has ever read the paper carefully.

The first experience with a test of my model of rumour spread was, in a way, traumatic but also, in a way richly instructive. This was the occasion of my first encounter with the post-war American military establishment. Assuming a large population and completely random contacts between its members, I deduced a relationship between the average number of persons contacted by each person hearing the rumour and the ultimate fraction of the population that will have heard the rumour. The process of random contacts turned out to be remarkably effective. It was shown, for example, that if the average number of persons contacted by each transmitter is two, fully 80 percent of the population will eventually have been contacted, and this fraction is virtually independent of the size of the population, which can be arbitrarily large. The same formal model applies to epidemics. In real life epidemics of this magnitude (pandemics) are rare, even though the numbers of persons contacted on the average per transmitter may be quite large. There are several factors that contribute to the limitation of epidemics: immunity, recovery, or death of infected, and, above all, the fact that contacts are not generally completely random. It stands to reason that most people contacted will be in the vicinity of the source, and this substantially reduces the rate of spread of a disease through a population. We can suppose that analogous factors enter the spread of information, behaviour patterns, and the like. I kept working on the problem, attempting to take such factors into account.

Before I got anywhere with it, a well known American sociologist visited me. He told me he had got a dramatic confirmation of my "theory" of rumour spread, referring to my model based on random target selection. He did an experiment in a large class, and the results fit the predictions of the "theory" almost perfectly. I tried to explain to my colleague that all he "tested" was the randomness of target selection, not the dynamics of rumour spread. He could have got the same results if he used markers instead of people. But I wasn't sure he knew what I was talking about.

Some time afterwards I received a report of another experiment performed by a team organized for that purpose. The experiment was funded by the Air Force with Air Force personnel taking part.

The cite was a town in Colorado. A piece of information was "planted" in the population, namely, a slogan used to advertise a brand of coffee: "Golden's coffee is as good as gold." A prominent announcement was made in the local paper to the effect that whoever repeated the slogan to an employee of a local market would receive a pound of this coffee while the supply lasted. The idea was to trace the spread of this information. It was assumed that people who became "knowers" would tell their relatives or friends. Accordingly, people who recited the slogan to get their coffee were asked to fill out a questionnaire, in which they were to indicate from whom they got the information and to whom they passed it on.

Why was the Air Force interested in funding an experiment of this sort? The most important reason, I later surmised, must have been that they felt obligated to dispose of moneys allocated to "research," of which there was God's plenty. If the money was not spent within a given period, it had to be returned.

Later the strength of this motivation became apparent in another context, namely, when Congress ordered operations in Cambodia to stop. Just before the deadline several tons of bombs had not yet been exploded. They were dropped on whatever targets were convenient, presumably so as not to let them go to waste. Fifteen thousand civilian casualties were reported.

The local Air Force command did not know much about experimental design, the use of controls, and the like. The rationale of this "investigation" was to see to what extent populations could be informed of whatever the Air Force wanted to inform them about if the usual media (radio, press, etc.) were not functioning, that is, if the spread of information had to depend on word of mouth. In other words, the experiment was to assess the effectiveness of person-to-person communication. But evidently this goal was somehow lost sight of. The local commander decided he was going to give the process a boost. Or, perhaps, he wanted to see how effectively the information could be spread by methods sometimes used by the Air Force, namely, by leaflets dropped from planes. Accordingly, he ordered the town covered with leaflets bringing glad tidings:

WANT TO GET A POUND OF COFFEE FREE?

JUST SAY "GOLDEN'S COFFEE IS AS GOOD AS GOLD AT ...
SUPERMARKET.

TELL YOUR FRIENDS THEY CAN DO THE SAME!!

To test the "effectiveness" of this campaign, more questions were added to the questionnaire, such as:

Did you pick up a leaflet? Where did you pick it up? Did you pass it on? If so, to whom? Did you tell any one what was on the leaflet?

As the leaflets were dropped, they were photographed from the plane, presumably to determine how they spread. A man was perched on the roof of a tall building, another on a mountain top overlooking the city to provide additional information on which way the leaflets went. Two cars toured the streets to see where the leaflets fell. The city was divided into districts and the numbers of leaflets that fell in each district recorded.

The report submitted to the sponsor included a section on "theory." Information, the reader was informed, can be transmitted in different forms, e.g., in writing, orally, by visible signals. It can be received first hand, second hand, etc. It can be transmitted accurately or distorted. People are motivated to transmit information in different ways. "Knowers" gain prestige. Desire to share knowledge with others plays a part. According to whether the news is good or bad, information will spread more or less rapidly, etc., etc.

I was shocked. I knew the designer of this "experiment," (the sociologist who had performed the meaningless class room experiment), a warm, good-natured human being, youthful in spirit in spite of advanced years and full of enthusiasm. But apparently he was deluded, seduced somehow. Here was a concrete manifestation of blossoming technolatriy (worship of technology) coupled with a sterile formalistic distortion of the so called "scientific method."

The Boulder fiasco brought home to me some sources of perversion of science. One was easy (too easy) access to funds, which encouraged wishful thinking about the significance of one's ideas. But the triviality of much of the research spawned by the availability of steady sources of funding was not what disturbed me most in the beginning of my life in academe. This could be easily rationalized. The projects provided opportunities for academic careers and so for the development of scientific talent where it existed (which might otherwise be extinguished for lack of ways of making a living). What I was most disturbed by was the co-optation of scientific talent by the military, by far the most generous dispenser of research funds. Never mind that the "spread of information" project was a scandalous waste of time and money. Many of the funded projects did much more damage. They led to sensational and far-reaching advances in techniques of mass destruction. They fed a euphoria generated by the sense of power in the self-designated "defence community." Coupled with this self-congratulatory euphoria was a rationalization of science in the service of war, prevalent in the scientific community itself, which spread from the natural to the social sciences.

Science and Values

Soon after I joined Rashevsky's group I came across a pamphlet by George Lundberg, a professor of sociology at the University of Washington, entitled *Can Science Save Us?* The motto introducing the pamphlet was a citation from *Alice in Wonderland*.

"Please, Puss," she [Alice addressing the Cheshire Cat] began, "can you tell me which way I ought to go from here?"

"That depends," answered the Cat, "on where you want to get to."

Science, argued Lundberg, can't tell us which way we ought to go. It can only tell us how to get where we want to go. Decisions about where we want to go are not scientific decisions. They are political decisions.

I disagreed categorically with this view of science, not to be sure, with Lundberg's view of how science currently functions (in this he was probably not too wide of the mark), but with his view of how science ought to function.

What science ought to be was the theme of my first book, *Science and the Goals of Man*. In this book I came to grips with two issues, which, I believe, should be illuminated from the point of view of the present role of science in human affairs. One is the relativity of values, an issue raised by cultural anthropologists of the American school. The other was the issue raised by Korzybski—the relation between the scientific mode of thinking and sanity.

The relativity of values is clearly related to the issue raised by Lundberg, namely, that the function of science in society must remain strictly instrumental: the question "What is to be done?" can be answered only with reference to the question how given goals are to be achieved. In this way, scientific cognition appears to be irrelevant to questions of ethics or morality. On the other hand, a cardinal principle of scientific cognition can be invoked to support the relativity of values, namely, the theory of relativity. In the light of careful analysis of what

happens in the process of observation and measurement, it turns out that even such apparently entirely "objective" data as the distance between two points or the time interval between two events are relative to the frames of reference of different observers if they are in motion with respect to each other.

The main theme in my first book, *Science and the Goals of Man* (1950) was based on the contention that the "relativists," in invoking relativity as a universal principle of cognition and, by implication of internalizing values, did not appreciate the full import of the relativistic paradigm. What the theory of relativity revealed was that *although* space and time intervals are relative to the frames of reference of observers, the so called *space-time*, a four-dimensional construct in which space and time are fused, *is* invariant for all observers. Thus it may be that while parochial value systems differ from each other, they may be reflections of a more general value system common to all humans. For example, the male Christian entering his place of worship removes his hat; the Jew keeps his head covered. Both express reverence but by different gestures. Among Europeans, the colour of mourning is black; among the Chinese white; but the underlying feeling of grief is common to both. Can it be that "relativity of values" can be transcended by the discovery of "ethical invariants," i.e., more general human values, of which those we are directly cognizant of are specific manifestations?

There have been attempts to list such supposedly universal human values. Abraham Maslow once offered such a list.

- (1) Every one feels a need to belong. That is, to be a member of a family, a peer group, an organization, a church, in short of a group whose members share the same values or pursue the same goals and hence have feelings of benevolence toward each other, based on giving and receiving support.
- (2) Every one feels a need to love some one or something.
- (3) Every one needs to have a feeling of self-respect, a feeling that he or she represents something worthy and that this worthiness is appreciated by others.
- (4) Every one has a need for growth, that is, expanding one's knowledge and awareness and progressive integration with others.

I agree with every one of these statements. But I have no way of knowing whether this agreement is simply a manifestation of the fact that Maslow and I belong to the same culture or, rather a subculture of what has come to be called "Western" culture. How can one be sure that if one traces the origins of different people's predilections, commitments, convictions, and aversions far enough, one will find that they are specific manifestations of these four basic needs? Or, for that matter, of any other set of general principles that can be justifiably called universal human values?

Attempting to answer this question, I chose for a starting point the assumption that all values stem from conceptions of what is true, what is good, and what is pleasant. The most obvious and, at times, glaring differences among people of different backgrounds are conceptions of what is pleasant. Having

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look outside culture
look into other species

reached a certain stage of maturity, we are generally not appalled by observing that what some people regard as delicious, others regard as repulsive. The notion of pleasure extends also to the notion of beauty. Here, perhaps, a conception of some universal aesthetic principle might be justified. But if such a conception turns out to be vacuous, it doesn't seem to matter much. In my estimation the statuette of the goddess of fertility discovered in the Wachau region of Austria, the Venus of Willendorf, as she has been named, doesn't hold a candle to Venus of Milos; but I can live with this sort of "relativity of values," as easily as with the knowledge that some people in Africa love to eat termites.

Differences in conceptions of what is "good," on the other hand, have had much more serious consequences. These underlie ideologies, and we have witnessed cataclysmic outbursts of violence attributed to clashes between ideologies.

Clashes between adherents of different religions or ideologies have also been attributed to differences of conceptions of what is "true." In fact, in the context of religions or ideologies, conceptions of what is "good" and of what is "true" are frequently confounded.

It seemed to me, therefore, that a recognition of universal standards of either truth or virtue or both could be of help in forestalling conflicts rooted in differences of conceptions of what is "true" or what is "good." Moreover, in view of the awesome destructive power of such conflicts manifested in modern war, it seemed imperative to find ways of preventing them.

Now it appears to me that conceptions of what is "good" are still largely conditioned by people's cultural background. Attempts to transcend these, for example, in numerous declarations of basic human rights or in distilling some general ethical principles supposedly underlying all religions, have become commonplace. But there is no assurance that these distillations are not themselves products of a particular culture. Indeed, they have most frequently been ascribed to the particular "Western" way of thinking and so are no more rooted in some universal principle than rules of etiquette or culinary traditions.

Nevertheless, it seems to me that conceptions of what is "true" can have a common foundation independent of ways of thinking or feeling induced by particular cultures. In fact, science provides such a foundation. The ways of thinking underlying scientific cognition are the same in Indonesia and in Iceland, in Uganda and in Uruguay, in Jordan and in Japan. The relativists can naturally reject this argument out of hand by pointing out that science is a distinct product of Western culture, and the fact that scientific thinking has to some extent penetrated cultures far removed from the Western is a consequence of Western imperialism. I don't agree. I believe that even if the so called "primitive" cultures were carefully nurtured and protected from cultural hegemony of the West, there would be no way, short of absolute intellectual quarantine (which could be imposed only by a ruling elite) of preventing the scientific view of the world from "infecting" any other world view and undermining some of its foundations. Of course, the repartee of the relativists can be immediately anticipated. Reference to a "ruling elite" carries a pejorative connotation and reveals a Western prejudice. What appears as a despotic ruling elite

to the Westerner imbued with Western ideas of democracy may appear as a wise, revered leadership to the people of a different culture. And so on. There is no way of resolving this controversy. The most each can do is to state his/her position sincerely and clearly. I have done this.

If this is indeed so, that is, if the adoption of the scientific mode of cognition in forming our conceptions of the world (and of ourselves) is ultimately inevitable, then a basis for resolving conflicts about what is "true" exists. Conflicts about what is "beautiful" or "pleasant" do not threaten humanity with extinction and can be resolved by simply recognizing the relativity of values in these contexts and developing tolerance based on that recognition. On the other hand, conflicts about what is "good" (essentially ideological conflicts) are fraught with danger. In view of the formidable destructive potential of massive organized violence, resolution of such conflicts appears to me to be a matter of survival of the human race. Recognition of the distinctive difference between so called instrumental and so called basic values suggests a way.

Instrumental values are those whose realization is regarded necessary for realization of fundamental values. Fundamental values are not further reducible. The distinction parallels the distinction between theorems and postulates of mathematics. Theorems are proved by reference to theorems proved previously. Infinite regress or circular reasoning is avoided by assuming some propositions to be "true" without proof. The same distinction is made in logic between defined and undefined terms. Terms (names, concepts, etc.) are defined with reference to other terms. Since the number of words in any language is finite, continuing this process will inevitably lead to circular definitions, for example, defining liberty as freedom and freedom as liberty. Thus, in any logically structured reasoning process, undefined terms must be involved which are assumed to be understood in the same way by every one.

As an example of reducing instrumental values to fundamental ones, consider a farmer who is asked why he sows wheat. He might respond by saying that he sows it in order to harvest it. Pushed further, he might reply that harvested wheat will produce grain, grain will produce flour, flour dough, dough bread, and bread will sustain his life. He may be either unwilling or unable to answer the question why he wants to sustain life. Grain, flour, dough, and bread represent instrumental values; life a fundamental one.

Now scientific cognition provides a way of gaining more or less reliable knowledge (at any rate knowledge more reliable than that provided by other forms of cognition) about the extent to which instrumental values function effectively. If sustaining life is a basic value, then a substance that helps cure a disease is an instrumental value. Whether a particular substance is, in fact, of value in curing a disease is something that can be determined by methods developed in science. Again, if one has a notion of a "good" society, then the extent to which different forms of social organization (e.g., democratic, hierarchical, autocratic) produce the "good" society can be assessed (in principle) by applying techniques of analysis, generalization, observation, deduction, etc., all of which are components of

scientific cognition. In other words, science provides more or less reliable methods of singling out, testing, and evaluating instrumental values, and in this way provides (in principle) ways of resolving disputes about what is "good" when "good" refers to effectiveness of ways of attaining basic values. Therefore, whether resolution of conflicts about what is "good" are in the last analysis resolvable by application of scientific cognition depends on whether universal basic values exist. The conviction that they do exist is an article of faith. It has the same status as postulates (that neither can nor need to be proved) in mathematics or undefined terms (that neither can nor need to be defined) in logic.

If this conviction is justified, that is, if differences about what is good turn out to be differences of opinion on how the fundamental values can be most effectively nurtured or realized, then scientific cognition (testing the effectiveness of instrumental values) becomes a basis of achieving unity amid diversity, that is taking the step from relativity of values to the recognition of invariant imperatives.

I discussed these matters once again in *Operational Philosophy*, published three years after *Science and the Goals of Man*. Along with scientific cognition, a central theme in both books was a philosophy of language strongly influenced by Alfred Korzybski.

General Semantics

Korzybski's philosophy of language rests on two assumptions. One views language as a specific survival mechanism of the human race; the other views it as a screen between a human being and reality.

Every plant and animal species survives for some time thanks to some sort of adaptation to its environment, which, in turn, determines its "life style." Among animals predators have their sharp claws, which enable them to catch and kill their prey. The animals preyed upon are equipped with fleet feet and a refined sense of smell, which enable them to become aware of predators and to escape them (sometimes). Some animals are expert swimmers, some are expert flyers, some expert runners. Man can do many of the things other animals can do but apparently not as well. He can swim but not as well as a seal; he can run but not as fast as a cheetah. His body is less specialized than that of most other animals. But there is one thing he can do far better than any non-human—talk. We don't call the sounds made by non-humans talking. But they do serve as signals in communication, along with gestures, including exuded odours. So non-humans have languages of sorts. Some bird species have been reported to have "vocabularies" of some hundreds of "words." Nevertheless, non-human languages are severely limited as instruments of communication. Animals can communicate only about "here" and "now." They cannot communicate about something that happened in the past, about something that is expected to happen in the future. They cannot communicate about an event that never happened or about a hypothetical event.

Consider a dog, an animal that can communicate to humans. A dog can "say," "Some one is coming!" He can say something like "I love you" or

something like "I'm sorry" (mostly by gestures). He can say something like "Give me some of what you are eating." But no dog can say, "When you were out, some one was trying to break in, but I chased him away with my barking." He cannot say, "Unless you give me a little of what you are eating, I shan't love you any more, and then *you'll* be sorry." All human beings who can talk can say things like that. That is, they can make statements about events removed in space and time, conditional statements, and statements about things that don't exist and about events that never happened. In short, they can lie.

The units of communication used by non-humans are called signals. They have direct relations to the things that they stand for. The units of communication used by humans are called symbols. Their connections to the things that they stand for are conventions. There is no necessary connection between a word and what the word stands for. In English the word for "word" is "word." In French it is "parole." In Russian it is "slovo." None of them is the "proper" way to say "word." Or, if you will, every one of them is equally proper. It is this lack of a natural connection between a word and what the word is supposed to stand for that enables human beings to invent words for hypothetical, not necessarily existing things, events, or relations. The principal idea of Korzybski's earlier book, *Manhood of Humanity*, is that man's symbolic language is his unique survival mechanism. It enables him to transmit knowledge gained by experience to those who did not have the same experiences and so makes possible learning by means other than direct experience. Moreover, it enables man to transmit experienced knowledge to future generations and so to make knowledge accumulate across generations. Korzybski called this process "time-binding."

Apparently time-binding is a unique characteristic of the human species. It immerses man in an environment entirely of his own making—a cultural environment.

The principal idea of Korzybski's only other book, *Science and Sanity*, is that symbolic language is by no means an unmixed blessing. Korzybski sees language as a sort of screen between man and the world in which he lives ("reality"). Man does not perceive reality directly. His conceptions of reality are formed by what he sees on the screen, and the picture on the screen is surely not a complete "map" of reality. In fact, no map can be a complete representation of a territory. Moreover, the picture on the screen need not even correspond to anything in reality. In sum, man forms conceptions of the world about him from what others (and also he himself) tell him about this world.

Sanity, according to Korzybski, is one pole of a spectrum. The other pole is insanity. The spectrum represents gradations between degrees of correspondence between a user's language and reality. The language of science is the language of sanity in the sense that the structure of the language of science corresponds most closely to the structure of reality. The structure of something reflects relations among its parts. For instance, the structure of a map reflects distances between locations and directions of some locations from others. If a map shows Kingston

percept
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roughly between Toronto and Montreal, its structure corresponds to the structure of the territory it represents. If it shows Toronto between Kingston and Montreal, it is a distortion of reality. The language of science corresponds most closely to reality that it is supposed to represent. Moreover, this map of reality is constantly being revised and updated and so made to correspond ever more closely to reality.

Between the language of science and the language of insanity, is the language that almost every one uses in every day life. The assertions about and descriptions of reality produced in this language are not, as a rule, constantly checked against direct experience (as the assertions and descriptions produced in scientific language are constantly checked by observation and experiment). Thus, a relation exists, according to Korzybski, between science and sanity.

It is now clear why language appears as a mixed blessing. Time-binding, a process generated by symbolic language, results in an accumulation of concepts, knowledge, ideas, attitudes, etc.—a map, whose structure may or may not correspond to the structure of reality it is supposed to represent. Whether it does or not, man “navigates” by this map. He may end up where he wants to get to or get lost in a forest or drown in quicksand or fall off a cliff.

Science and the Goals of Man and *Operational Philosophy* represented a crystallization of my conception of science as primarily a reflection of collective sanity—collective because it relates truth to common experience, a reflection of sanity, because the structure of its language corresponds most accurately to the structure of reality. This conception of science, therefore, contains an ethical component. Identifying awareness of truth with shared experience serves to integrate humankind. All other bases of integration, for instance, institutionalized religions, identification with a national, ethnic, or racial group, have both an integrative and a divisive component. The latter tends to split people into “us” and “them.” Integration based on shared scientific cognition is not limited by such a divisive component.

Sanity and the Cold War

As I said, I was a member of the Communist Party from November, 1938 to December, 1941, a fact which I looked forward to stating in public when, as I expected, I would be called to testify before a U.S. congressional committee investigating “un-American activities.” I resigned from the party in accordance with the party policy of rendering unqualified support to the U.S. war effort. After the war I was invited to rejoin. By that time the “line” had changed again. Earl Browder, who had coined the slogan “Communism is Twentieth Century Americanism” was replaced by an old timer, William Z. Foster. The hew “hard” line was transmitted to all the parties of the West by Maurice Thorez, the French Party boss. No trace remained of the United Front. Strict orthodoxy, primacy of the class struggle, etc. were the order of the day.

I had become especially sensitive to demagoguery. I couldn’t stomach it. I read Lenin’s *Materialism and Empiriocriticism* and dismissed it as balderdash. I counted the number of “isms,” with which Lenin labeled the views of people he castigated

as tools of the bourgeoisie on the “philosophical front.” There were fifty-two. The compulsion to condemn everything and every one by sticking labels on him, her, or it struck me as the most conspicuous manifestation of confounding verbal maps and reality. It was this degradation of language converted to a demagogue’s weapon that finally led to my break with orthodox Communism. My “Kronstadt” was the Lysenko affair.⁴

I applied the same standards to anti-Communism. This too was a torrent of demagoguery. In 1950 a colleague of mine, Alfonso Shimbelt, Gwen, and I wrote an article entitled “Sanity and the Cold War.” It embodied our common political outlook at that time. The article included a fantasy scenario embodying a renunciation of totalitarianism by the Soviet Union and its unilateral withdrawal from the arms race. In fact, it was an anticipation of Gorbachev’s “conversion,” which Gwen and I witnessed forty years later (Al died young). The way we saw it then, the sudden refusal on the part of the Soviet Union to cooperate with the U.S. in nurturing the global war machine would cut the political ground from under the U.S. military establishment.

At the time the prospect appeared to us to be entirely in the realm of fantasy. But it was not on that ground that Hayakawa refused to publish the article in *ETC.: A Review of General Semantics*. As co-editor I felt it was entirely proper for me to argue in favour of publication, for example by pointing out that Cold War rhetoric was a perfect example of language pathology—identifying verbally constructed images with reality; of completely polarized two-valued orientation, of demagogic manipulation, and so on, in short a textbook case study of language pollution on the global scale.

But there was no way to engage Hayakawa in a discussion where participants had to take sides. I was fond of debate, but Hayakawa hated every form of polemics. He often spoke contemptuously of people who were ever aching to “prove something.” Human communication was to him a venture in sharing, not in “competing,” as he conceived all forms of dispute. So I changed the subject. I recalled his saying one time something about the impossibility of “resigning from a culture.” I asked him what he meant by it.

This put the conversation back on track. He said publishing our article would be tantamount to resigning from our culture. I asked him what “culture” he had in mind. If he meant the “mainstream” American culture, I said I didn’t have to resign from it, because I never belonged. Then he told me about the “Nisei blues,” the longing of American-born Japanese⁵ to “make it” in the WASP world, which he certainly did. He escaped internment during the war. He was a celebrity, author of a best seller. I remember listening silently and gloomily. It cost great effort to refrain from leveling with him the same way, to tell him that he was mistaken in assuming that I experienced something akin to “Nisei blues” (a Jewish immigrant boy longing to make good in the WASP world—we both married WASPS). I had a deep affection for Hayakawa. So while in those days I felt little compunction in riding roughshod over some one’s feelings, I held back. I was not to break with him until twenty years later when he supported the

Vietnam war and tried to prevent a paper of mine from being presented at a conference of the International Society of General Semantics.

Gwen, Al Shimbel, and I submitted "Sanity and the Cold War" to *Measure*, the University of Chicago magazine edited by Robert Hutchins, where it was published.

General System Theory

I mentioned the rise in prestige of science in the U.S. following the dramatic finish of World War II. Suddenly an abstruse mathematical theory was revealed as a source of knowledge conferring awesome power on its possessor. Generous support for developing a theoretical infrastructure for "high tech" spilled over into related "new fields" such as the theory of games, cybernetics, and information theory which all made their "public debut" within a year of each other.⁶

Among these was so called "general system theory," not nearly so widely publicized as the others but attracting considerable attention among academics in search of an intellectual base for counteracting the increasing isolation of specialized disciplines.

General system theory is usually associated with Ludwig von Bertalanffy, an Austrian biologist, and he, in turn, traced the underlying idea to the ongoing controversy between so called "vitalists" and "mechanists." The former maintained that life processes could never be completely explained in terms of laws (e.g., of physics and chemistry) governing the behaviour of non-living matter; the latter insisted that they could.

At one time the vitalists insisted that living tissue contains substances that do not occur in non-living matter. They called these substances "organic," whence organic chemistry got its name. This position had to be abandoned in 1828 when Friedrich Wöhler succeeded in synthesizing urea, an "organic" compound. This was, perhaps, the first of successive "defeats" suffered by the vitalists forcing them to retreat, one is tempted to say, to previously prepared positions.

In the first decade of our century, H. Driesch, a noted vitalist maintained he had shown experimentally the unique faculty of living tissue which he called "equifinality." He cut an embryo of a sea urchin in its early stage in two, put the two halves in separate containers with a nutrient medium and showed that both halves developed into normal sea urchins. Had the process of development been "mechanical," Driesch argued, the two halves would have developed into two halves of a sea urchin. That they didn't shows that the development was governed by a "goal," a "purpose," if you will, to become what the bit of living matter was destined to become—a sea urchin. He called this governing principle "equifinality," and asserted that it characterizes only the behaviour of living tissue.

Bertalanffy purported to refute this argument by pointing out that "equifinality" is characteristic of the behaviour of certain "open systems," that is, systems that exchange matter and/or energy with their environment, of which

living organisms are certainly examples but not the only ones. Thus, the distinction between "open" and "isolated" systems becomes a fundamental one in theoretical biology. Indeed the apparent violation of the Second Law of Thermodynamics by living organisms,⁷ sometimes cited by vitalists in support of their positions is not really a violation, since the law applies only to isolated systems.

The intellectual climate in the late forties was receptive to discussions about new "paradigms" in science, probably also a by-product of the dramatic consequences of applying "esoteric" theories (e.g., theory of relativity, quantum mechanics, mathematical theory of information) to new marvels of technology (harnessing of atomic energy, computerized thinking, automation).

Central to general system theory is the concept of a system as a portion of the world that somehow maintains its identity in spite of impacts of the environment that might destroy it. The most conspicuous manifestation of this faculty is the ability of living systems to resist encroachments on their integrity, continued functioning, and development. The designation "general" refers to the generalization of the notion of "system," particularly of "living system" to subsystems of an organism (cells, organs, tissues) and to supersystems containing individual organisms as subsystems, in the case of human aggregates, families, work teams, tribes, nations, the international system. The psychologist or the psychiatrist may single out the "psyche" as the focus of attention and seek to discover the way this "system" develops and maintains its integrity or, on the contrary, disintegrates, as in mental disease, and the way it influences other components of a human being.

It was probably this problem area that motivated Roy Grinker, a prominent specialist in psychosomatic medicine and psychiatrist at the Michael Reese Research Hospital in Chicago to organize an ongoing conference on general system theory in Chicago, in which I was invited to participate. We met twice a year. Some of us were permanent members of the conference; others were invited as specialists to participate in sessions devoted to some one or another aspect. It is in these sessions that I met Karl Deutsch, a "system-oriented" political scientist, whose book *The Nerves of Government* impressed me as an excellent example of combining suggestive metaphor (regarding governments as organisms) with rigorous analysis. I was later to engage in other analyses of this sort.

Grinker's conferences inspired a similar program at the University of Chicago, organized by James G. Miller, then chair of the psychology department, and Ralph Gerard, a physiologist, later a co-founder with Bertalanffy, Kenneth Boulding and myself of the International Society for General System Research. The idea spread to the Soviet Union, where a year book *Sistemnye Issledovaniya* (systemic investigations) soon made its appearance in a format similar to our *General Systems* the yearbook of the Society, edited by Bertalanffy and myself. Common interests led to my continuing contact and collaboration with the Institute of System Research in Moscow which have continued to the present day.

My sojourn in Chicago came to an abrupt end in 1954.

Witch Hunt

In 1950 the U.S. went to war against Communism in Korea, and the McCarthy "witch hunt" went into high gear. It was, of course, only a pale reflection of the paranoia that gripped the Soviet Union. It induced something like nausea rather than terror, but it nevertheless filled with despair people who hoped for a resumption of the promise of the New Deal interrupted by the war. Financial support for the University of Chicago (suspected of harbouring subversion) was drying up. To revive it, some assurance of loyalty to "American values" had to be shown. That meant that Hutchins had to go. A skilled money raiser unstained by either "intellectual elitism" or softness on Communism replaced him. The U.S. senatorial Jenner Committee (one of the committees investigating "un-American activities" on campuses) was welcomed at the University of Chicago.

A group formed including people who were likely to be called to the hearings. The big issue was whether to invoke the Fifth Amendment to the Constitution (the right to refuse to testify on grounds of self-incrimination). The Communists argued for invoking it. Most of the others argued against. They intended to answer bravely all questions except those that demanded incrimination of others. In doing so, they risked the charge of contempt of the Committee. But this was evidently an additional inducement, at least for some, to refrain from invoking the Fifth. It was a chance to publicly denounce the assault on American liberty and to suffer martyrdom on that account. I envisaged that fate for myself. Even though in answering the question "Are you or have you ever been...?" I intended to say, "I am not, but I have been," this is as far as I would go. I would steadfastly refuse to incriminate others.

It never came to this. I was not called. Three of my colleagues, members of the Committee on Mathematical Biology, were called. One of them was Al Shimbel, co-author of "Sanity and the Cold War." He did not invoke the Fifth Amendment, but hard as the committee tried, they could get nothing interesting out of him. Another colleague, who had done research in ballistics during the war was asked whether he was engaged in espionage at that time. He turned to an attorney and asked him whether if he answered that question in the negative, he would be obliged to answer other questions. The attorney said he would. Then my colleague invoked the Fifth Amendment and was excused from further testimony. He was fired. So was a third member of the Committee on Mathematical Biology. It was done delicately. Their contracts were simply not renewed. I never found out why I was not called.

With Hutchins' departure the atmosphere at the University of Chicago became oppressive. So when I was offered a year's fellowship at the newly established Ford Foundation Center for Advanced Study in the Behavioral Sciences in California, I accepted and resigned my assistant professorship.

In August 1954, we (Gwen, our two year old daughter, and I) put some belongings into our old Plymouth (Gwen's dowry) and set out for California. We did not intend to come back.

NOTES

1. *Othello*, Act III, Scene iii.
2. Later director of Energy Research and Development in the Federal Energy Office.
3. A logarithmic function of a variable increases with the variable at a decreasing rate; a linear function increases (or decreases) at a constant rate.
4. In 1921, sailors in Kronstadt, the principal Russian naval base, revolted against the Communist dictatorship. The suppression of the uprising by massive executionism led to resignation of many American Communists from the party. The notorious Moscow trials and the Hitler-Stalin pact on the eve of World War II led to further resignations. The successive disillusionments of American Communists, mostly intellectuals, led to the dictum "Every American ex-Communist had his Kronstadt. Mine was the 'purge' of geneticists from Universities and research institutes for 'heretical' views inconsistent with the official philosophy of science. Trofim Lysenko became the ideological watch dog of biological science. For me that was the final straw.
5. Hayakawa was born in Canada but became an American citizen.
6. John von Neumann's and Oscar Morgenstern's *Theory of Games and Economic Behaviour* (second edition) and Norbert Wiener's *Cybernetics* were published in 1947; C.E. Shannon's and W. Weaver's *A Mathematical Theory of Communication* in 1948.
7. In the course of development, living systems become more organized. Some vitalists misinterpreted this process as a violation of the Second Law of thermodynamics, which supposedly requires a constant increase in entropy in physical systems. Such increase is characteristic of systems isolated from their environment, which living systems obviously are not.